THURSDAY, NOVEMBER 23, 1876

FERRIER ON THE BRAIN

The Functions of the Brain. By David Ferrier, M.D., F.R.S. With numerous Illustrations. (London : Smith, Elder, and Co., 1876.)

I.

THIS is in many respects an important work. Full of experimental facts and theoretical suggestions, clearly and forcibly written, it is important as a contribution to our knowledge (and our ignorance) of the functions of the brain. The reader must not misunderstand my parenthesis as an epigram. That we are ignorant of brain-function is undoubted ; and this ignorance is sustained and fortified by the "false persuasion of knowledge" which prevents search in other directions. Such false persuasion of knowledge will be deepened by Dr. Ferrier's work—all the more because of its merits, *if* the conclusions maintained there are erroneous, and the conceptions which determine them are unphysiological ; and on both points I am inclined to judge affirmatively. There is something seductive in the precision of his statements and the unhesitating confidence with which only one side of a question is presented. The reader is easily led captive by a writer who has no hesitation. Add to this the many difficulties which stand in the way of controlling by experiment the experimental data, and the indisposition of most men to undertake the labour of verification, and we may foresee that physicians and psychologists will eagerly accept this work as an authoritative storehouse of material for their speculations. They will see how its "facts" harmonise with their own pet errors. They will interpret clinical observations or psychological facts by its conclusions. Already we have seen various theories invoking the Hitzig-Ferrier views ; and when nerve-cells of a larger size than usual are found in a particular region of the cortex they are straightway declared to be motor-cells, because the region is said by Hitzig and Ferrier to be motor, while the existence of these cells is adduced in confirmation of the hypothesis respecting the region !

In view of the too-probable precipitation in adopting the conclusions of this work, we cannot do better than emphasize the warning with which the author closes his Introduction :—

"We are still only on the threshold of the inquiry, and it may be questioned whether the time has even yet arrived for an attempt to explain the mechanism of the brain and its functions. To thoughtful minds the time may seem as far off as ever."

The volume opens with an elementary sketch of the structure of the brain and cord, followed by a short chapter on the reflex functions of the cord, with passing reference to Pflüger's view of its sensory functions, and to Goltz's experiments against that view. Then follows a chapter on the medulla oblongata as a respiratory and vaso-motor centre ; and one on the general relations of the mesencephalon and the cerebellum. After full, yet brief accounts of what is taught respecting the effects of removing the cerebrum, the mechanism of equilibration, the muscular sense, the function of the semicircular canals, vertigo, co-ordination of locomotion, and finally the mechanism of emotional expression, we are brought to the main topic of the book—the functions of the cerebrum and basal ganglia. Let a word, in passing, be also given to the excellent chapter in which the psychological aspect of the cerebrum is treated.

Rich as the work is in facts and suggestions, it is so deficient in the indispensable correctives of counterfacts and arguments, that the reader must be cautioned against accepting any position unless elsewhere verified. Partly because, from long occupation with his subject, Dr. Ferrier has become unable to see it in any other light than that of his own hypothesis, and therefore doffs aside all counter-facts and counter-arguments as not really significant ; partly, perhaps, because his memory has let slip what must have entered into his knowledge ; from one cause or another there is a disregard of counter-evidence, which, in a second edition, I should seriously urge him to rectify. Let me cite examples.

In arguing against the sensory functions of the spinal cord, the experiment which he urges as decisive is Goltz's well-known experiment on the insensibility of the brainless frog to pain. I formerly (NATURE, vol. ix. p. 84 pointed out the defect in logic, which concludes from the fact that under certain conditions a brainless animal is insensible to pain (equally to be said of animals with brains), therefore it is altogether without sensibility. Pain and sensation are so far from being equivalent terms that not only are the great mass of our sensations without pain, but some cannot even be exaggerated into pain. Dr. Ferrier probably did not read the article in which I answered Goltz ; but did he also overlook the article in the *Journal of Anatomy* for November, 1873, or the same article in the *Studies in the Physiological Laboratory of Cambridge*, Part 1, where Prof. Michael Foster showed by decisive experiments that the facts observed by Goltz had another interpretation ? Again, is it possible that Dr. Ferrier has never been made to hesitate in assigning the optic thalami and corpora striata respectively as the integration of sensory and motor centres, by the observations and experiments which show that sensibility sometimes persists after total destruction of the optic thalami, and that paralysis does not always follow destruction of the corpora striata ? One such observation would be decisive against these localisations. But Dr. Ferrier neither disproves the facts nor suffers them to disturb his views of the functions of these ganglia. Finally, there is an experiment by Dr. Burdon Sanderson which, as I shall presently show, cuts the very ground from under Dr. Ferrier's feet—yet this he does not even mention. He probably overlooked its significance ; at any rate he leaves his readers without the advantage of knowing that there is such a fact.

That this disregard arises from no unfairness, but simply from the onesidedness which comes from preoccupation with certain views, is evident in the way he equally disregards his own counter-evidence. A notable instance is the first assigning the occipital lobes as centres of organic sensations on the faith of observed absence of such sensations when the lobes were removed, and then citing a case of complete recovery of such sensation five days afterwards, and instead of recognising this as decisive against his hypothesis, still persisting in maintaining it.

Thus much on what may be called the "personal equation." Another and more serious source of the misleading effects of the book seems to me its following the

increasingly popular but thoroughly unphysiological conception of Localisation. Were not the current notions respecting organ and function very chaotic, and were not the indispensable artifice of analysis mistaken for more than an artifice which demanded rectification by synthesis, we should marvel to witness so many eminent investigators cheering each other on in the wild-goose chase of a function localised in a cerebral convolution. I will not, however, dwell on this point here, because it is one which would require a long discussion. It is only mentioned as a general *caveat*, and as leading up to the main question of cerebral excitation.

In 1870 Hitzig and Fritsch startled the scientific world by announcing that the universally accredited notion of the brain not being excitable was an error. The most eminent experimenters had declared that mechanical, chemical, and electrical stimuli were utterly powerless to excite the grey matter; and many a writer pointed to the paradox of the chief organ of sensation being *insensible*. We may here note another example of the common confusion of sensibility with pain; the brain was said to be "insensible" because no cutting, burning, pricking, or galvanising of it yielded evidence of pain; whether other evidence of sensibility might have been present was not asked. The utmost the experiments could prove was that the brain was not excitable by these abnormal means, though excitable by the very different normal means of peripheral stimulus. And even this conclusion Hitzig and Fritsch upset, by demonstrating that there were certain regions of the cortical substance which were excitable by electricity, as proved by the movements following such excitation; and the other "non-excitable regions" they inferred to be also excitable, though in another way, namely, by the production of sensations (*Vorstellungen*).

This was an epoch-making discovery. Experimenters in Germany, Italy, England, Switzerland, France, and America, quickly verified it, although differing among each other both as to the particular facts, and their interpretation. Among these followers the chief place must be assigned to Dr. Ferrier, both for the extent and the precision of his results; accordingly the names of Hitzig and Ferrier are usually coupled in speaking of the new hypothesis that various motor centres are located in particular spots of the cerebral cortex.

Although I have called it an epoch-making discovery, because I believe it will open a new track for the anatomical and physiological interpretation of the nervous mechanism, which will one day enable us to follow the whole pathway of stimulation, instead of—as at present—leaving us with the vague conception that "somehow" the cerebrum determines movements by setting the motor apparatus in action, I do not think that the hypothesis of motor centres in the cerebrum is tenable; nay, more, I *do not think that Hitzig and Ferrier have proved the grey substance to be excitable.* It is one thing to admit that the brain is excitable, another to admit that the excitation so effected is effected by calling into activity the special property of the grey substance. We do not consider the fauces to be the centre of vomiting, although tickling the fauces will be followed by retching. We do not consider the centre of laughter to be located in the sole of the foot, because tickling the sole causes laughter. Something more is needed; and it is precisely this something more which the Hitzig-Ferrier hypothesis has yet to find, namely, the anatomical connection of the so-called centre with the motor apparatus.

Has any proof been adduced that the electrical stimulus *first* acts on the cortex, and *then*—by the stimulation there produced—on the white substance, which in turn acts on the motor ganglia? *None that withstands criticism.* Knowing as we do that if the cortex be removed, or destroyed, the electrical stimulus nevertheless on reaching the white substance determines the same movements which had previously been determined when the stimulus was applied to the cortex, we may fairly ask: What proof is there that the current does not pass *through* the cortex (as through any other conducting medium) without exciting its activity? That it does simply pass through the cortex is probable on two grounds: (1) only the electrical current causes an excitation; mechanical and chemical stimuli have no such effects, because they cannot pass through the cortex to reach the white substance; (2) it is a well-known law that the propagation of neurility, *unlike* that of electricity, takes place only at insensible distances: if the nerve be divided, and the two cut surfaces be brought into the closest possible contact, there is still no propagation of the excitation from one surface to the other; whereas electricity passes freely *across* the cut surfaces. Now here Dr. Burdon Sanderson's decisive experiment, formerly referred to, comes, as I said, to cut the very ground from under the Hitzig-Ferrier hypothesis. "If that part of the surface of the hemisphere which comprises the active spots is severed from the deeper parts by a nearly horizontal incision made with a thin-bladed knife, and the instrument is at once withdrawn without dislocation of the severed part, and the excitation of the active spots thereupon repeated, *the result is the same as when the surface of the uninjured organ is acted upon*" (*Proceedings* of the Royal Society, No. 153). Here the interruption caused by the incision, while it must have completely prevented the propagation of *neural* excitation, did not prevent the propagation of the *electrical* current. Clearly therefore the simple passage *through* the cortex will explain all the effects of electrical stimulation. Clearly therefore some other proof is needed before we can assign the motor effects to an excitation of the cortex. The arguments of Dr. Ferrier (pp. 135-6) are all set at nought by Dr. Sanderson's experiment; and on the physiological and histological views now adopted I do not see how Dr. Sanderson's experiment can be brought into agreement with the motor centre hypothesis.

Nevertheless, although I say that the preliminary fact of excitation of the cortex is not proved by Hitzig and Ferrier, I do not myself doubt that fact, although my reasons will sound so paradoxical that I must wait for another article to give them expression.

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(*To be continued.*)

GREEK AND LATIN PHILOLOGY

Baur's Philological Introduction to Greek and Latin for Students. Translated from the German by C. Kegan Paul and E. D. Stone. (London: King and Co., 1876.)

WITH the publication of Jacob Grimm's "German Grammar" the comparative study of language entered upon a new period of existence. Bopp and the